Anisotropic metric-based mesh adaptation for ice flow modelling in Firedrake

This document provides a point-by-point response to the comments raised by Reviewer #2.

Response to Reviewer #2

We thank the reviewer for the review! Original comments are written in black, while our responses are provided in blue.

Dundovic and co-authors present a new ice flow model that represents significant progress in adaptive mesh methods applied to ice flow problems. The key novelty is in an iterative adaption technique that does not simply produce a time-sequence of meshes, each based on the ice sheet state at a given time, revises the mesh at each time step to explicitly obtain optimal solutions at later times. The paper also presents some generally applicable ideas in that type of mesh refinement in plain language (with some illustrative math) and I enjoyed reading those parts in particular. Overall a good paper that should be published and fits GMD well. My comments are minor.

General Comments

The paper claims at a couple of points that adaptive mesh (AMR) methods are underused in ice flow modelling and should be used more on the basis of this paper. These are not justified (or technical) points and should be removed (e.g last paragraph of the conclusion). There are several adaptive mesh ice flow models: Ua (often with AMR), BISICLES (always), ISSM (sometimes), UFEMISM, in use on large scale, realistic problems today (the paper references these codes). The model here is tested only on a toy problem with a tiny domain (MISMP+), and even then, halves the domain. There are also well-regarded uniform resolution models (PISM, CISM and others). Clearly, many modellers find AMR not utterly compelling even when it is available. This paper will not change that, because the performance improvements noted relative to fixed-in-time meshes are modest.

We agree that this was not an accurate assessment, especially since we indeed then go on to cite these various works. We have therefore removed the sentence in the introduction which claims that mesh adaptation methods are underused in glaciology. However, in response to the other reviewer's comment 84, we end our Conclusions section with a sentence stating that "more research is needed to investigate the effectiveness of these methods in larger-scale and less idealised models."

There is no mention of the expected or observed asymptotic rates of error estimate convergence with 1/N. Figure 9 shows error estimates, which look to me to be $O(N^{-1})$ in panel (a) and perhaps tending to $O(N^{-2})$ in panel (b). Figure 9 could be modified to show those reference rates, and some discussion made – what is leading order error do you expect from your discretization? The discontinuity Tb (or rather, in dTb/dx in this case, I think) likely reduces you to first order in space. It also sounds from your description (compute the velocity, then advance the thickness) as though you are first order in time.

- *We agree that this should be explicitly mentioned. We have modified the text to do so (at the end of section "Mesh adaptation strategy" and around this figure). However, we have decided not to include the $O(N^{-1})$ and $O(N^{-2})$ convergence rates directly in the figure. The other reviewer has commented (see their Recommendation paragraph) that the figure is too complicated, and has asked for it to be broken down into two separate figures. Since this figure already shows results from five different sets of experiments (one for each choice sensor fields + uniform meshes), we did not want to again complicate it further.*
- *The Euler approximation to the coupled model scheme is indeed first-order in time, as we discuss further in comments 11 and 13 below.*

1. L84 H of Ω -> H(Ω) ?

   Thank you; this was indeed clumsy. We removed "of Ω" and now simply say: "... spatial discretisation in terms of a mesh*, $\mathcal{H}$, which..." Similar change was made in line 122.*

2. L119 ρ and r_1 are adequate to describe the mesh parameters, but cannot 'fully support anisotropic mesh adaption' – for that you need the other things you describe (sensor fields etc). Removing the sentence would retain the meaning of the paragraph.

Thanks for pointing this out. We modified the sentence to now instead read:"Thus the metric-based approach introduces anisotropy in the resulting mesh." as suggested by the other reviewer (comment 22).

3. Figure 1. I would like to see a zoom of the GL regions – that way it is easier for the reader to see what is meant by the signs of r_1 & r_2

   Good idea, thanks. In response to the other reviewer's comments, we expanded this figure to show three different metric fields. On two of the corresponding adapted meshes we zoom in onto the grounding line and on one we zoom in onto the shear margin.

4. L126: given that the unit size is impossible, why not say 'approximately unit size'?

   Done.

5. L134-135. I see what you are saying there, but I think you can say more -in some regions the elements are stretched along x, in other along y.

   Thank you for the suggestion. We have expanded this sentence to be more specific and have modified the figure to include a zoom-in on the grounding line and shear margin regions of adapted meshes to better demonstrate this variation (as described in comment 3 above).

6. L147: both h- and p-adaption are 'active and impactful'. Agreed, but citation needed. Also, p-adaption is in many problems the more efficient approach, you might comment on that.

   We have briefly expanded on the pros and cons of both p- and h-refinement and cited the recent work of Kirby and Shapero, 2024 alongside the already-cited Cuzzone et al., 2018.

7. L170 : say a bit more about Cst. To me it seems to be space complexity, controlling the mesh density in a 'uniform' fashion in space (crudely, you can obtain a mesh with 2X Cst by splitting each element in two, I know that is not what you actually do). But how is it a time-complexity?

   We have modified the text for clarity. It now reads: "The space-time complexity provides an estimate for the average number of mesh vertices in the entire mesh sequence $\{\mathcal{H}_i\}_{i=1}^{N_a}$. The number of vertices, however, may vary between meshes, as they are distributed in both space and time among individual meshes in the sequence..." That is to say, $\mathcal{C}_{st}$ is a constraint corresponding to the average number of vertices of the entire mesh sequence in time-dependent mesh adaptation. In contrast, $\mathcal{C}_s$ is a constraint corresponding the number of vertices of a single mesh.

8. Fig 2 and section 2.5: I did not find Figure 2 helpful – only partly understanding it by reading the (quite wordy) text of section 2.5, rather than the figure illustrating the text. I would like to see these algorithms presented as pseudo-code, which would make section 2.5 more accessible.

   As suggested by both reviewers, we removed the figure and presented the global and hybrid algorithms as pseudocode. We also tidied up the text as described in comment 47 of the other reviewer (removed duplicated statements, rearranged a few sentences...).

9. Section 2.6 Interpolation between meshes. Indeed, this is important, at least in principle. It is for the sake of conservation and avoidance of (a type of) numerical diffusion when transferring thickness/temperature between meshes that we use the more restrictive block-structured meshes in BISICLES (which relies on a long-standing AMR library, Chombo, originally developed for shock problems in hyperbolic PDEs, where numerical diffusion is anathema). You might note this.

   Thank you for sharing this; we included it in the first paragraph of the section.

10. L318-319 Not quite. Viscous (longitudinal and lateral) stresses matter in at least some parts of ice streams, close to the GL, or where basal traction is low, or in shear margins.

    Thank you; we added a new sentence that closely follows your explanation. We also cite Greve and Blatter, 2009.

11. L344 – this is what makes me think your methods is first order in time. Is that the case?

    The Euler approximation to the coupled model scheme which alternates between solving the diagnostic and prognostic equations is indeed first-order. We have now explicitly stated this.

12. L349 / eqn 12. Is this really done just to smooth the GL? In which case, you are following the approach of Leguy 2014 as well as AMR. Or is it in fact because eq 12 thought to better represent the physics than the discontinuous laws? The pure 'Weertman' problem (P -> infinity) is harder (Tb is discontinuous across the GL), than the modified form (Tb is continuous across the GL, at least when P is given by the formula in L354* but dTb/dx is not), and the best justification for not addressing it is physics. Also, should β^2 be β^{-2} in eqn 12. So that $Tb = \beta^2 |u|^{1/3}$. * You say that L354is the formula for water pressure under floating ice, but you only need such a formula in the basal traction under grounded ice. Or do you compute P there differently?

    - *The motivation to use some form of the Schoof sliding law was indeed just to smooth the GL, as we describe in the text. We already cite Leguy et al., 2014 in the introduction and we have now cited it again here. And just in case the question was referring to why we use a bespoke form of the Schoof sliding law: it is because Firedrake doesn't support hypergeometric functions, which are required for the original Schoof sliding law implementation (Shapero et al., 2021).*
    - *Thank you, β^2 was indeed a typo. Changed to β^{-2}.*
    - *Sentence in L354 related to pressure was indeed confusing. We rewrote the equation to now explicitly consider the grounded and floating parts. We also clarified the text.*

13. L356 'second order' – but only if you supply it with velocity at t + dt/2, rather than t? Seems not to agree with L344.

    The icepack implementation of the Lax-Wendroff scheme for transport is indeed second-order and can be found here: https://github.com/icepack/icepack/blob/master/src/icepack/solvers/flow_solver.py#L348. In the code, the second derivative term $\partial^2 h/\partial t^2$ is represented in weak form as a sum of flux contributions in the interior of the domain and at boundaries. However, we have decided to drop "second-order" from this sentence to avoid further confusion. As noted in comment 11, the Euler approximation to the coupled model scheme is indeed first-order.

14. Fig. 4 / sec 3.2 I am not convinced that you are seeing convergence here. As you note earlier, you need to show a pattern of successively closer solutions, which might apply to the 1km-500m-250 sequence, but you need one more refinement (125m) to look convincing. The gap between 1km and 500 m looks much larger (10x?) than the gap between 500 and 250 m, but once the leading order term in truncation error dominates, you hope to see the gap successively halving (for $O(N^{-1})$) / quartering ($O(N^{-2})$) etc.

    We completely agree; thank you for pointing this out. We ran a further experiment with the 125m resolution and added the result to this figure, as well as mentioning it in the text. This now (properly) shows that results have indeed sufficiently converged, so the rest of our results remain unaffected.

15. L368 'The Ice1 experiment starts from *an* initial state'. Just a typo

    Thank you; we corrected this.

16. L382 – why this time-step in particular? CFL on your finest mesh?

    The intention is indeed to satisfy the CFL condition. We now explicitly state this both in the uniform and adaptive mesh experiments. The timestep size $1/24\,a$ is in fact considerably smaller than the CFL condition requires on the finest 125m resolution mesh (approximately $125\,m/1644\,ma^{-1} = 1.824/24\,a$). We took an even smaller timestep to satisfy the CFL condition on adaptive meshes that could have even finer local resolution, since we do not prescribe bounds on minimum element lengths.

17. L389 – all simulations were run in serial – comment on this, was that just to measure CPU time, or is your method not yet suitable for parallel runs.

    We modified the sentence in this line and in line 545 (of the unrevised manuscript) to now say that it is possible to run simulations in parallel, but that we indeed ran them in serial for the purposes of CPU time measurements.

18. Fig 5 captions – define the symbols used on the axes e.g deviation in V.A.F ($||\Delta V_f||_\infty$)

    Thank you; we have done this in Figure 5 as requested, and also in Figure 4.

19. Fig 6 (and 7) Zoom in on the GL – as it is I can see there is some difference in your meshes, but not in detail

    We have done this in both figures.

20. L 487: 'Ice bends etc' – in SSA? There is no adjusting to buoyant forces in SSA, which assumes hydrostatic balance. The horizontal stretching occurs because all the driving stress from the front / local slopes is resisted by viscous stresses and hence results in horizontal longintudinal strain-rates.

    Thank you; we simplified this sentence to avoid using the term "hinge zone" and what you pointed out in the comment. Instead we simply say "... on the ice shelf near the grounding line."

21. L496 'basal stress is discontinuous across the GL' – not according to eqn 12. P -> 0 as you approach the GL from the upstream side (or is P computed differently from the formula in L354?). Also L500: as u->0, P -> ∞ eqn 12 becomes $|Tb| = |\beta^2 u^{1/3}|$ - the discontinuity you are seeing at the inflow boundary seems like an arithmetic artefact, with u/|u| being undefined (but also irrelevant since |Tb| = 0)

    Thanks for pointing this out.

    - *The basal stress is indeed not discontinuous, so we now instead say that it rapidly diminishes across the grounding line. Similar modification was made in Line 509 of the unrevised manuscript.*
    - *We agree with your assessment of the refinement at the inflow boundary. However, we have decided to drop this sentence for conciseness. It is a minor point in an already quite wordy section.*

22. L575: This paragraph seems spurious.

    We agree. The main point of this paragraph is in the final sentence, which was further clarified two paragraphs down, so we decided to completely remove this paragraph.

23. L597: I disagree. You have demonstrated progress, but it is up to the reader to decide whether they should adopt the methods.

    We have removed this sentence. Detailed response given under the General Comments section.

References

Cuzzone, J. K., Morlighem, M., Larour, E., Schlegel, N., & Seroussi, H. (2018). Implementation of higher-order vertical finite elements in issm v4. 13 for improved ice sheet flow modeling over paleoclimate timescales. *Geoscientific Model Development*, *11*(5), 1683–1694.

Greve, R., & Blatter, H. (2009). *Dynamics of ice sheets and glaciers*. Springer Science & Business Media.

Kirby, R. C., & Shapero, D. (2024). High-order bounds-satisfying approximation of partial differential equations via finite element variational inequalities. *Numerische Mathematik*, 1–21.

Leguy, G., Asay-Davis, X., & Lipscomb, W. (2014). Parameterization of basal friction near grounding lines in a one-dimensional ice sheet model. *The Cryosphere*, *8*(4), 1239–1259.

Shapero, D., Badgeley, J., Hoffmann, A., & Joughin, I. (2021). Icepack: A new glacier flow modeling package in python, version 1.0. *Geoscientific Model Development Discussions*, *2021*, 1–34.